Biogeosciences Discussions

Biogeosciences Discuss., author comment AC2
https://doi.org/10.5194/bg-2021-277-AC2, 2022

Reply on RC2

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Author comment on "Fire in lichen-rich subarctic tundra changes carbon and nitrogen cycling between ecosystem compartments but has minor effects on stocks" by Ramona J. Heim et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-277-AC2, 2022

We thank the Reviewer for the helpful and constructive comments which will help to greatly improve our manuscript. We are prepared to incorporate all raised points as suggested below and are confident that we can meet and successfully address all issues. Our answers and proposed text improvements are written in italic and bold below each comment.

Anonymous Referee #2

The manuscript by Heim et al. reports on C and N stocks and their stable isotope composition in various ecosystem compartments (soil organic matter, aboveground vegetation including lichen and vascular plants) in three large fire scars (between 12 and 44 years post-fire) and adjacent control sites; in Western Siberia. Only d15N data on soil samples are missing (due to low soil N concentrations, although I would think this is quite feasible from a technical point of view).

The authors' main conclusions are that

(i) total ecosystem C and N stocks were not significantly affected by fire.

(ii) soil C and N stocks were not affected. Soil make up the majority of ecosystem C and N stocks.

(iii) vegetation was affected, mainly due to a reduction in the lichen layer, which takes a long time to recover.

(iv) a few trends in d13C and d15N – but those are not well explained, see further.

Personally, I would stick to the first 3 (which are related, obviously). Given that the soils in these systems are relatively poor in organic C (from less than 1 to slighty over 2% on DW basis), these conclusions are in line with expectations. The interpretation of the isotope data is not conclusive and too speculative.

Thank you for this helpful assessment - more on this point in the answers to the following comments!

Overall, I would mostly encourage the authors to take out the more speculative sections, keep the manuscript and conclusions to what can be unambiguously demonstrated, i.e. fire affects aboveground biomass, and not belowground OC stocks in a system with rather mineral soils, and as the biomass presents a small fraction of ecosystem OC stocks, those are not strongly affected. The isotope data are intriguing, but I do not feel much can be drawn from them at this stage.

Good point. The fact that there is hardly any isotope data regarding fire effects in tundra ecosystems available makes a discussion quite difficult, we acknowledge this and therefore keep this section now much shorter. However, we are convinced that our data offers potential for discussions and interpretation in future studies of this topic and would thus prefer to leave it in. Nevertheless, we agree to largely follow your suggestions in substantially shortening these sections and limiting our interpretation.

Carbon stable isotope data

-abstract L 11-13: "This could be related to …": this is not conclusive, and your data do not really allow you to draw anything firm from this.

We will remove this conclusion.

-Figure 2: use small delta symbol, not capital delta on y axis label

We present differences in the "small" delta on the y axis (D = d_2 – d_1) to facilitate identifying differences. In this case to our knowledge a capital delta is commonly used.

-The mechanism behind the decrease in d13C in plants and lichen is not well understood. I am not very convinced on the suggestion that lower d13C in local atmospheric CO2 due to the fire would be the cause. This would imply a higher CO2 concentration, from increased mineralization – but the soil OC stocks suggest there is no enhanced mineralization (at least not observed as a decrease in C stocks). Such local variations in CO2 and d13C-CO2 are typically observed in closed canopy systems. Without actual data demonstrating local gradients in d13C-CO2, I would not make this suggestion too explicit. The authors refer to Dawson et al. (2002) and Lakatos et al. (2007) in this context – but neither of these papers mention anything about fire and its effect on local d13C-CO2 years after the actual burning.

Yes, this is a good point as well. We have to discuss those ideas more carefully. The text in lines 239ff is now:

"Why $\delta^{13}C$ in vascular and lower plants on fire scars is decreased in our study is relatively unclear as variations in $\delta^{13}C$ are usually complex and not straightforward to interpret (Dawson et al., 2002). Therefore, our findings could not be related with certainty to a process described in literature. One reason for the decreased $\delta^{13}C$ might be the lower ^{13}C content of the CO_2 in the ambient air of fire scars. A lower ^{13}C content of the CO_2 can be explained by increased decomposition rates (Dawson et al., 2002; Lakatos et al., 2007). However, we could not detect a decrease in C stocks in our data that would allow the

assumption of increased mineralisation."

Alternative explanations for minor shifts in plant d13C (water availability or sources, interactions with nutrients, ..) are not considered (although with the data at hand, one would not be able to make a strong case for a precise mechanism).

Yes, we thought about other alternative explanations as well, however, it seems that the knowledge on these complex relationships seems to be relatively small for further reasonable explanations.

Increased water availability, for example, is common in post-fire permafrost landscapes (Holloway et al. 2020). Water availability may indeed be a better explanation for the observed shifts in d13C in vegetation here, as we found a lower d13C on burnt areas in vascular and lower plants. This agrees well with the frequently observed negative correlations of indicators for humidity and tree ring d13C (e.g. Holzkämper et al. 2012), as d13C reflects stomatal conductance as affected by moisture availability or drought stress. Dawson 2002 states that "But unlike in vascular plants, delta tends to increase with water limitation in nonvascular plant taxa (Williams & Flanagan 1996, 1998) ".

Our soil moisture data, however, does not support this, which might be due to the timing of sampling (soil moisture is also probably the most variable parameter in space and time).

Holloway, J. E., Lewkowicz, A. G., Douglas, T. A., Li, X., Turetsky, M. R., Baltzer, J. L., & Jin, H. (2020). Impact of wildfire on permafrost landscapes: A review of recent advances and future prospects. Permafrost and Periglacial Processes, 31(3), 371-382.

Holzkämper, S., Tillman, P. K., Kuhry, P., & Esper, J. (2012). Comparison of stable carbon and oxygen isotopes in Picea glauca tree rings and Sphagnum fuscum moss remains from subarctic Canada. Quaternary Research, 78(2), 295-302.

-Hence, I also find that conclusions such as "they [=lichen] strongly reflect environmental changes, such as increased soil respiration, after a fire", since the data presented do not show direct evidence for increased soil respiration.

Yes, this is a good point. We will remove this rather uncertain discussion from our conclusions in lines 263ff.

-The explanation offered for the soil d13C data is also too speculative in my opinion. Linking such a small difference to increased temperatures (no data are presented to back up an increase in temperature), and I feel that Ehleringer et al. (2000) is not adequately interpreted here: microbial communities do not have an inherent preference for 13C-depleted organic matter- that's not what Ehleringer et al. conclude. Microbial biomass appears to be slightly enriched in 13C, yes – but again, given that the soil C stocks do not appear to change, why invoke higher mineralization (and thus, a higher contribution of microbial biomass) ?

Yes, thank you again for expressing your concerns. The explanation is indeed quite speculative, and we will remove it from the discussion.

Nitrogen stocks

The scenarios described in Section 4.1.1. are quite speculative – possibilities, but nothing conclusive.

Yes, we discussed this part more cautiously:

"A possible explanation for this pattern may be linked to enhanced competition for nitrogen among vascular plants, which increased during post-fire succession (Bret-Harte et al., 2013; Heim et al., 2021). Unburnt plots were dominated by lichens, which obtain large parts of their nutrients from the atmosphere (Asplund and Wardle, 2017) and thus did not compete for available soil N. Therefore, vascular plants at unburnt plots may have relatively more available N.

Lichens reflected long-term impacts of fire on N cycling. We found high N concentrations on burnt plots of the youngest fire scar. The disappearance of this effect with time since fire might be related to the fact that younger lichens generally contain more N (Kytöviita and Crittenden, 2007).

Soil N concentrations were only increased on the oldest fire scar and in the upper soil layer. This pattern is less likely linked to the temperature-mediated increased microbial activity, as the soil temperature in the oldest fire scar recovered to control levels (Heim et al., 2021). Rather, this might be explained by the increased cover of vascular plants, which produce more root exudates, have symbiotic nitrogen fixation, and easily decomposable falloff litter (Maslov et al., 2018; McLaren et al., 2017)."

Statistical analysis: The approach used by the authors is outside my comfort zone, but I do not understand why they follow this road (posterior distributions). It would be good to shed some light on why this is helpful, and why not simply use the actual measured data. I would personally prefer to see the actual data presented in the ms, and the predicted values in the supplement.

We used data analysis in a Bayesian framework, as the sample size of our study is too small to include several random factors which we have to include to correct for the study design. On the base of the measured data, we can therefore not state anything regarding significance.

Minor suggestions

-species names should be in italics throughout

Thanks. Will be corrected throughout.

-soil d15N data: given the N concentrations (0.05 % DW approximately), I don't see why this is not possible – 20 mg of dried sample should provide 10 µg N for analysis, which is largely sufficient for good d15N data.

We did several tests to capture a reliable ^{15}N signal. However, the very large masses of sample needed to obtain good ^{15}N data (indeed 20-30 mg) caused several problems. Firstly, large tin capsules got stuck in the autosampler, which made manual sample insertion necessary. After solving this it turned out that such large sample masses lead to very high amounts of ash residues in the combustion tubes. This affected the quality of the combustion significantly, causing higher yields of NOx which could not fully be reduced back in the reduction reactor. Therefore, ^{15}N values of these samples had very bad reproducibility and made very frequent cleaning of the oxidation reactor necessary. Due to the insufficient quality of data and as little data is available from other studies to provide room for comparison and interpretation, we decided to skip d^{15}N.